

decide upon the exact terminations of the tibia, as they are confused by the joints connecting with the other members, with the result that "the personal equation" largely effects the result, and I find myself frequently more or less out of agreement with both Chapman and Metcalfe. This difficulty will in a measure explain, even if it does not excuse, some of the divergences which may be discovered in the following details.

These explanations become necessary when I approach the question of the new British species separated by Chapman, and Tutt, premising that *Masonia edwardsella*, Tutt, and *Fumea germanica*, Chapman, are not British.

(To be continued.)

What is a Species?

By the late DR. T. A. CHAPMAN, F.R.S., F.Z.S., F.E.S.

This is an abstract question and may, according to the way in which it is approached and the kind of answer that will be accepted, be a very easy or a very difficult one. What is wanted, however, is not a reply to an *abstract* question, but a reply to the *concrete* one, Are two groups of individuals all of the same species, or is each group of a species distinct from the other; and by what criterion shall we arrive at a just conclusion?

To follow the evolution of our idea of species I may be allowed to revert for a moment to the abstract question. I may, as having a pre-Darwinian memory, refer to the answer that still found most acceptance fifty years ago, and even later; not that our present answer was wanting, but that it wanted the support that the Darwinian explanation gives. The answer was, that a species is a group of individuals all descended from separately created ancestors, presumably such a pair as was preserved by Noah, and consequently all related to one another and unrelated to any other individuals.

How far have we travelled from this conception? To frame a definition of our present idea of a species as nearly as may be in the terminology of the older one, I might say that a species is a group of individuals which might all be supposed to be descended from a single pair with distinctive characters, and within a moderate (though indefinite) period.

We suppose a species usually to arise by the isolation of a section of a pre-existing species, variation and selection producing the necessary modifications. And the new species, it is often pointed out to us, is descended, *not from a single pair*, but from the whole (possibly very large) isolated section. This is true in a sense, but *quâ* distinct species, the modified older idea of an ancestry limited to a single pair is more nearly true. The species is distinct, because it has, under an altered environment, acquired by variation and selection certain new distinct characters.

These distinct characters, preserved and cultivated because valuable under the new circumstances, may have originated in the isolated section frequently, and got lost by sheer accident, but one day an individual possessing the required character (no doubt usually in a very moderate degree) left progeny, a fair proportion of which inherited the character, and from this basis the new character in its full develop-

ment became finally built up. Probably each feature characterising the new species originated in this way from *one* individual, and so there is only one ancestor for each distinctive character, or correlated group of characters. If however we select one distinctive character of the species, we have descent from *one* ancestor, as well no doubt as from many others, but this *one* ancestor is distinctive from everything represented in the present species.

It may even be a question, how far infertility with related species may be due to the final dominance of the strain of this important primary individual. When a species is dividing into two or more distinct ones by isolation, there must be a longer or shorter period during which they are still *one* species, and finally a date at which they are perfectly distinct. Mathematically we might say, there must be a particular moment before which they are *one*, after which they are two species. Biologically, or at least experimentally, this is not the case, there is a period during which the matter remains doubtful. Probably if we had a sieve suitable for sifting the individuals, we should find that some individuals were already distinct, that others were less differentiated; still more probably, if each individual could be sifted, some of its elements would be found distinct, others still in common. This latter is probably the case in species that are, and have very long been, somewhat widely distinct species. The common elements, however, are unable to declare their affinity in face of predominance of the immeasurable elements, since all must develop more or less together.

If this be so, absolute distinction for practical purposes declares itself long before complete distinction of all the elements of the germ plasm is achieved. But going in the other direction, must we not equally conclude that we may have distinctness of the character that we look for in species, in many elements of the germ plasm, long before they have any influence in producing infertility between individuals that possess them and those that do not.

Following out the ideas thus suggested, specific distinction might be defined as immiscibility of germ plasm, and distinct species would be those where individuals possess a certain (possibly variable in different families and genera) definite proportion of such immiscible germ-plasm.

It would result from this that there may be between two races the greatest variation in the amount of specific distinctness, and that this indefiniteness is a matter of fact and not of the personal equation of the observer. This might be taken to be arguing in a circle, were it not that instances of this vagueness of specific distinctness are not rare phenomena, but abound on all hands.

What I have said gives my ideas, not of species but of what constitutes specific differentiation.

My idea of species is to take something out of this that will be useful for practical purposes.

I conclude that the races of peas experimented on by Mendel and Bateson are all of one species, but I should postulate that they possess some elements of specific distinctness. *Smerinthus ocellatus* and *populi*, on the other hand, are distinct species, but still possess some elements that are specifically identical.

Trying to frame a general test that would separate these pairs of forms that are always regarded as of one species, from those that are

always regarded as two, should incidentally give some guidance (which after all is only some useful rule for ordinary rough purposes) on the doubtful and intermediate races, the conditions force us to take one of the degrees of miscibility. What this degree shall be, must in the end, be such a practical one as will appeal to the ordinary field naturalist, is in fact dependent on the personal equation, and the personal equation must be that, if we can ascertain it, of the man who is practically dealing with specific forms in life.

Such a test seems to be that if the two forms placed together in a suitable environment, mix readily to a common and average form, then they are one species, if not, they are distinct. A common and average form must be interpreted broadly, and would include alternative and mutually exclusive variations, and so on. If the two forms appear to mix freely, and the mixed form reverts at once to one or other of the parent forms (apart from selection due to unsuitable environment) then the species are distinct.

Such a test is however a counsel of perfection, the time and pains necessary to apply it to a given case will rarely be held to be remunerative, and what we really have to do is to bring in once again the personal equation, and decide, as best we may, from our other knowledge of the two forms, what would be their behaviour were such a test applied.

Entomological Notes from Putney for 1922.

By H. DONISTHORPE, F.Z.S., F.E.S.

January 11th.—*Forficula auricularia*, out on footpath.

February 23rd.—*Coccinella bipuncta*, on a fence.

February 28th.—A ♂ of *Apis mellifica*, on the wing.

March 8th.—*Vespa vulgaris* ♀, on the wing.

April 15th.—*Coccinella 7-punctata*, on a fence; *Vespa vulgaris* ♀, on the wing.

April 23rd.—*Bombus* sp.? flying in my garden.

April 29th.—*Geotrupes sylvaticus*, a very small brightly coloured specimen walking on Putney Common.

May 20th.—*Phymatocera aterrima* (Solomon's Seal Saw-fly), flying in my garden.

May 22nd.—*Atomaria linearis*, flew into my study window.

May 23rd.—*Phlogophora meticulosa* (the Angleshades Moth), on a fence.

June 2nd.—*Pygaera bucephala* (Buff-tip), and *Lucanus cervus*, on a fence in my garden.

July 18th.—*Lucanus cervus* ♀, on a fence in Oakhill Road.

July 20th.—*Rhizotrogus solstitialis*, quite a small specimen at large in my garden. This is the first time I have noticed this beetle at Putney, though no doubt it is not uncommon. In July, 1920, however, I let loose in my garden a number of specimens which I had brought up from Cornwall; and the specimen in question may have sprung from them.

August 10th.—*Calathus flavipes*, on the pavement.

August 24th.—*Pyraus atalanta* (the Red Admiral), in the garden; *Tenebrio molitor*, on the pavement.